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ART. I.—1. *Essai sur la Température de l'Intérieur de la Terre.* Par M. L. CORDIER, Membre de l'Académie Royale des Sciences, de l'Institut, &c. Lu à l'Académie des Sciences, des 4 Juin, 9 et 23 Juillet, 1827. pp. 84.

2. *Essay on the Temperature of the Interior of the Earth.* By M. L. CORDIER, &c. Translated from the French, by the Junior Class in Amherst College. Amherst, Mass. August, 1828. 18mo. pp. 94.

3. *Considerations on Volcanoes, the probable Causes of their Phenomena, the Laws which determine their March, the Disposition of their Products, and their Connexion with the present State and past History of the Globe; leading to the Establishment of a New Theory of the Earth.* By POULETT SCROPE, Secretary of the Geological Society. London, 1825. 8vo. pp. 270.

RESPECTABLE geologists of the present century have been nearly as much distinguished for their skepticism, in regard to theories of the earth, as their predecessors were for their credulousness. Lecturers on the subject, indeed, chiefly perhaps that they might not seem to be wanting in system, have given a sort of poetic exhibition of their faith in some geological hypothesis—in most instances, in this country, the Wernerian. The more practical geologists, however, have devoted themselves exclusively to the observation of facts, exhibiting even a fastidious avoidance of hypothesis. One of the most

distinguished clusters of them in Europe, for example, the London Geological Society, have chosen the following, from Lord Bacon, for their watchword;—'Quod si cui mortalium cordi et curæ sit, non tantum inventis hærere, atque iis uti, sed ad ulteriora penetrare; atque non disputando adversarium, sed opere naturam vincere; denique non belle et probabiliter opinari, sed certo et ostentive scire; tales, tanquam veri scientiarum filii, nobis (si videbitur) se adjungant; ut omissis naturæ atriis, quæ infiniti contriverunt, aditus aliquando ad interiora patefiat.'

It was agreeable to human nature, that when men of logical minds saw the inadequacy of existing theories to explain the phenomena, they should abandon themselves to unreasonable skepticism in regard to any attempt to theorize. And the great number and zeal of the adherents of one of these hypotheses, the Wernerian, has increased this skeptical tendency among men, who saw that it was merely names and authority that kept up the illusion. While, however, the Neptunian system has been losing ground, we have for several years observed, even among the most cautious, a leaning towards the leading idea of Hutton, that internal heat has been the grand agent in the formation of our globe. A great variety of facts, brought to light by modern science, has tended to produce this change of opinion. Among these may be named the occurrence of more than two hundred volcanoes in all parts of the globe, and the identity of the lavas they have ejected; also, the existence of numerous extinct volcanoes, and the gradual passage of their lavas into every variety of trap rocks, thus establishing the identity of their origin; also, the occurrence of granite under precisely the same forms as trap rocks, and similarly intruded among other rocks, producing like dislocations and overturnings. Experiments also have been made which show that rocks may readily be made to assume a crystalline form, from a state of fusion; while few, if any facts, support the idea that they could have resulted from chemical solution in water. Observation has likewise shown that there is a constant radiation of heat from the earth towards the heavens; and that the earth is gradually cooling. But the argument more direct and conclusive than any other, perhaps, in favor of internal heat, is derived from certain experiments that have been made in deep mines, and other excavations, showing that the heat rapidly increases as we descend. This fact has but recently at-

tracted the attention of philosophers; and even up to this time, it has been considered as by no means established. To bring together all that is known on this subject, to discuss the merit of the observations by pointing out local causes of error, and to establish the truth of the general principle, constitute the objects of M. Cordier in the Essay, named at the head of this article.

'In the first part of the work,' says he, 'I shall discuss the merit of the experiments hitherto published, on subterranean temperature, and of the consequences which have been drawn from them; and shall give an account of corroborating experiments to which I have devoted myself. In the second part, I shall exhibit in detail, some direct experiments which I have attempted, while pursuing a new system of observations, and shall enumerate the immediate consequences which appeared to me necessarily to result from my researches. In the third part, I shall point out the principal applications to the theory of the earth, and as connected with this subject, shall present summarily many new geological observations.' p. 8.

The author finds that the number of mines, in which experiments on this subject have been made, in France, England, Switzerland, Saxony, Peru, and Mexico, is more than forty; and the number of observations about three hundred. They were made, some upon the air, some upon the water, and some upon the rock in excavations; and at depths varying from 127 to 1700 feet. A critical examination of these experiments occupies forty pages of M. Cordier's Essay; and we recommend this part of his work to any who may be disposed to impute the high temperature of mines, and other excavations to local causes. They will find, we presume, that the author has fairly and ingeniously estimated the amount of influence exerted upon the temperature by these local causes; and probably, also, they will find several sources of error here examined, which they have never thought of, and which would not be apt to occur to any man not thoroughly conversant with mining operations. The author derives from this examination the following conclusions.

'1. If we reject a certain number of observations as too uncertain, all the rest indicate, in a manner more or less certain, that there exists a remarkable increase of temperature, as we descend from the surface of the earth towards the interior. It is reasonable then to admit this increase.

'2. The results collected at the Observatory at Paris, are the

only ones that can be certainly depended upon, for obtaining a numerical expression of the law of this increase. This expression gives 51 feet as the depth which corresponds to an increase of one degree in the subterranean temperature. And we would remark in passing, that according to this result, the temperature of boiling water, under the city of Paris, would be at the depth of 8212 feet, or about a mile and a half.

'3. Among all the other results, a small number only, afford numerical expressions of the law sought for, sufficiently approximate to be taken into account. These expressions vary from 104 to 24 feet, for one degree of increase; their average in general indicates an increase more rapid than has been hitherto admitted. Their evidence has so much the more weight, as embracing the results of many series of long continued observations.

'4. Lastly, in grouping together, by countries, all the results admissible on any principle, I am led to present a new and important idea, to wit, that the differences between the results collected at the same place [different places? Trans.] are referable not solely to the imperfection of the experiments, but also to a certain irregularity in the distribution of subterranean heat in different countries. p. 50.

In order to avoid the local causes of error, to which most experiments upon subterranean temperature had been subject, M. Cordier performed several himself in the coal mines of France. We have not time to detail the precautions which were used; although every one who attempts similar experiments (and we hope they will be attempted in this country), should be acquainted with them. Suffice it to say, they were as complete as science and experience could make them. The following are the inferences from these experiments, and all the others detailed by the author.

'1. Our experiments fully prove the existence of an internal heat which is natural to the terrestrial globe, which depends not on the influence of the sun's rays, and which increases rapidly with the depth.

'2. The increase of subterranean heat in proportion to the depth does not follow the same law throughout the whole earth. It may be twice or even thrice as great, in one country as in another.

'3. These differences are not in a constant ratio to the latitude or longitude.

'4. Finally, the increase is certainly much more rapid than has heretofore been supposed; it may be as great as 27, or even 24 feet, for a degree in some countries. Provisionally, however, the mean must not be put lower than 46 feet.' p. 70.

These inferences we consider as legitimate; nor can it be any longer doubted that there is a very sensible and even rapid increase of heat as we descend into the earth. The establishment of this fact, we consider as constituting the principal value of M. Cordier's work. Further observations may indeed modify these conclusions, and bring to light others of an interesting character. But any longer to doubt the fact of an internal heat in the globe, not derived from local causes, nor from the rays of the sun, we consider as unreasonable skepticism. This fact being admitted, every philosophical mind is almost irresistibly led to make several theoretical inferences of a highly interesting character. If the heat increases to the centre of the earth, at the rate of one degree for 46 feet, the excessive temperature of 3500 degrees of Wedgwood's pyrometer, equal to 450,000 degrees of Fahrenheit, must exist there. And a temperature sufficient to melt all known rocks would exist at a depth of little more than 60 miles. Indeed, from many geological facts, M. Cordier is of opinion, that such a heat exists at a much less depth. He infers that the whole mass of the globe, with the exception of this crust, less than 60 miles in thickness, is at present composed of melted lava, similar to that which is so frequently thrown from volcanoes, which he regards as the vent-holes of this vast subterranean furnace,—the safety valves of our globe.

We are aware that it is, by some, thought to be 'a prodigious leap, from these experiments in the small way, to the igneous liquidity of the central mass!' The true state of the argument appears to us to be this. So far as we have yet penetrated into the earth, we find the temperature to increase one degree for every 46 feet. At this rate, all the rocks would be melted, long before we arrive at the centre of the globe. Now M. Cordier may ask, what reason have we to suppose the heat does not increase to the centre, at the same rate as it does for 1700 feet? If any one doubts it, let him show at what point the heat ceases to increase. But he looks around him, and finds 200 volcanoes on the globe, pouring out melted rocks, in just the same state as he supposes them to exist in the interior of the earth. It has been proved, moreover, that this melted lava, in many instances, at least, is ejected from beneath the primary rocks. It strikes us, that almost any man, under such circumstances, would be apt to believe that this volcanic lava, before his eyes, was no other than a portion of the ignited fluid matter, which

his experiments on subterranean temperature had led him to suspect, might exist in the earth. And when he saw the spheroidal figure of the earth, and that the organic remains of northern latitudes were evidently the products of a tropical climate, he would be very likely to feel a stronger conviction of the correctness of his first inference.

Nor would his faith in this conclusion probably be shaken, were he asked, as he is asked by an anonymous correspondent of one of the scientific journals of our country, whether, 'if the earth was, at the beginning, highly heated throughout, it would cool in that uniform ratio assumed by him; so that the remaining heat may be represented by a four-sided pyramid whose sides are isosceles triangles? Ought not the remaining heat to be represented by a four-sided pyramid whose sides are the areas of Gothic arches with sides nearly parallel towards the base? The latter would certainly be the true figure for representing the remaining heat of a red hot cannon ball, after it had been suspended by a chain, until its superficial heat should be so far reduced that it could be borne by the hand.'

Without entering into the mathematical considerations involved in these inquiries, M. Cordier might reasonably doubt, whether it were safe to infer the precise ratio of refrigeration in so large a body as the earth, covered, as it is, with an envelope many miles in thickness, composed of materials which scarcely conduct heat at all, from the manner in which an iron ball, a few inches in thickness, and an excellent conductor of heat, gives off its caloric. At least, he would probably think it safer to trust to the indications of central heat and fluidity, exhibited by volcanoes, the figure of the earth, &c. than to any physico-mathematical inference of this kind. Or, even if he granted the correctness of this writer's positions, he might say that the red heat of iron, admitted by him to exist at the earth's centre on his own principles, would be but little inferior to a temperature which would answer all the essential conditions of his hypothesis.

This idea of internal heat and fluidity, constitutes the fundamental element of a new Theory of the Earth; though in fact, it is very analogous to the views of Hutton. The supposition is, that the whole globe was originally in a state of fusion, and that from the beginning, it has been gradually cooling by the radiation of its heat; thus adding new beds of primary rocks to

the interior of its crust, while upon its surface the secondary deposits have been forming by the action of water. According to this view, the lowest and the highest rocks are the newest, and primary, as well as alluvial rocks, are still forming.

If we admit the leading idea of this system, the most difficult phenomena in geology, as well as in some other branches of science, are explained by it with great ease. It supposes a vast volcanic agency to have been in operation from the creation;—much more active and powerful in early times than at present. Such a cause would explain most satisfactorily all the anomalies of the trap rocks, and of granite; also, the inclination of rock strata in general, with all their overturnings, contortions, and dislocations; also, the intrusion among all classes of rocks of metallic and other veins; also, the elevation of our present continents from the bottom of the ocean. The most important of these applications we shall now proceed to make with all possible brevity.

The explanation of volcanic phenomena by this theory is satisfactory, and even beautiful. 'They appear,' says M. Cordier, 'to be the simple and natural effect of the cooling of the interior of the globe;—an effect purely thermometrical.' The contraction of the refrigerating crust subjects, as he supposes, the internal fluid matter to an immense pressure, by which it is forced out through the volcanic vents; and he calculates that this force cannot be less, in some cases, than 28,000 atmospheres. He makes the following curious estimates on this subject.

'At Teneriffe in 1803, I calculated as nearly as possible, the amount of matter ejected by the eruptions of 1705 and 1798. I performed the same operation, in respect to the products of two eruptions, yet more perfectly isolated, which exist in the extinct volcanoes of the interior of France; to wit, in 1806, those of the volcano of Murol, in Auvergne; and in 1809, those of the volcano of Cherchemus, near Izarles, at Mezin. I found the volume of matter in each eruption, to be much less than one cubic kilometre, or 1308044971 cubic yards. From these data, and others of the same kind, which I have obtained at other places, I feel justified in taking the volume of a cubic kilometre, as the extreme limit of the product of eruptions in general. But such a mass is very small in relation to the whole earth. Applied to its surface, it would form a bed, which would not be one 500th of a millimetre in thickness. More definitely, if we suppose the mean thickness of the crust of the earth to be 62,1 miles, a contraction of

this envelope, which would shorten the mean radius of the central mass one 494th of a millimetre (one 12694th of an inch), would be sufficient to produce the matter of one eruption.

'Proceeding upon these data, if we suppose that the contraction alone produces the phenomenon, and that over all the earth five eruptions take place yearly, we shall come to the conclusion, that the difference between the contraction of the solid crust of the earth, and that of the internal mass, would not shorten the radius of that mass more than a millimetre (,03937 of an inch) in a century; if there are but two eruptious per year, it would require two centuries and a half to shorten the radius as much. We see that, in all these cases, an action extremely small is sufficient to produce the phenomena.' p. 83.

Admitting this general explanation of volcanic phenomena, it is easy to see how it would account for 'the identity of circumstances that characterize volcanic action in all parts of the earth; also for the very great reduction in the number of volcanoes since the origin of things; also, for the diminution in the quantity of matter ejected at each eruption; also, for the almost exact resemblance in composition, of the products ejected at each geological epoch; and also, for the small differences that exist among lavas that appertain to different epochs.'

The striking features of volcanic action have excited a strong curiosity to know its origin. In every age, accordingly, hypotheses have been almost as numerous as philosophers. Our own age, so fertile in every species of intellectual creation, has produced its full share; and scarcely do we take up a scientific journal, without meeting with some new attempt to explain the origin of these igneous phenomena. The repetition of these efforts shows that they are unsatisfactory. Indeed, we must be pardoned for saying, that so far as the probability of the hypothesis is concerned, we do not apprehend that modern philosophers, until the appearance of the theory above explained, had made much advance upon the Greek and Roman poets; who described volcanoes as the forging-shops of the Cyclops, and the prisons of the giants who rebelled against Jupiter.

A very ingenious hypothesis, however, to explain volcanic phenomena, has of late been proposed and ably defended both in Europe, and in this country; and for a time, at least, it seems destined to be a rival theory to that of Scrope, Cordier, and others, whose outlines we have given above. This refers volcanoes to the action of water upon the metallic bases of the

earths and alkalies in the interior of the earth, where these bases are supposed still to exist in a metallic state. But more of this theory before we close.

Earthquakes, we believe, are now almost universally referred to the same source as volcanoes; or rather the two phenomena are considered as merely different exhibitions of the same power. If the supposition of central fluidity from heat be admitted, it is reasonable to suppose, that as the internal mass continues to cool and increase the thickness of the earth's crust, a part of the matter would be decomposed and form gases, as in the coagulation of lavas. These will be continually struggling to get vent; and being urged from place to place by inequality of pressure, along the probably irregular interior surface of the earth's envelope, will produce all the capricious phenomena of earthquakes. And when these gases have acquired sufficient expansive power, by their accumulation, to break through this envelope, or when they have found their way to some volcanic vent, they will drive out a quantity of the fluid matter with which they are surrounded, in the form of lavas; and thus the eruption would put an end to the earthquake, an inference that corresponds exactly with facts.

The diversities of climate found on the globe, have excited no small degree of interest among philosophers, to determine their cause. But the failure of explanation here, has been as conspicuous as in the case of volcanoes. Let us apply to this subject the supposition of central heat and fluidity.

There is reason to suppose that the thickness of the earth's crust varies very much in different countries. The thinner that crust, the more easily will the internal heat pass through it. In such a place, therefore, we should expect that the climate would be warmer, than in one situated upon a thicker part of the envelope. Only admit, then, that such inequality in the thickness of the crust exists, and we see why it is that the climate is so different, in the same latitudes, on different continents.

On this principle the envelope of the liquid fiery mass in the earth ought to be thicker in this country than in Europe, since the mean temperature of our climate, in the same latitudes, is considerably lower than in Europe. It is an interesting inquiry whether experiments confirm this inference from the theory. If the crust be thicker here, the temperature, as we descend into the earth, ought to increase at a slower rate than in Eu-

rope. The only conclusive experiment however, with which we are acquainted, that has been performed in the United States on subterranean temperature, is that of Mr Disbrow, made at Brunswick, New Jersey, and quoted in the note appended to the translation of Cordier's Essay. In boring for water at that place, he found the temperature of a spring, that issued from the strata at the depth of two hundred and fifty feet, to be fifty-two degrees of Fahrenheit, while that of another spring, at the depth of three hundred and ninety-four feet, was fifty-four degrees. This would indicate an increase of one degree of heat, at that place, for seventy-two feet in depth; whereas in Europe, according to M. Cordier, it is only forty-six feet. The experiment of Mr Disbrow appears to have been a very satisfactory one; yet we ought not, probably, to place much dependence on results derived from a single trial of this kind. We trust however, that no opportunity will be lost in future, for multiplying such observations in this country.

'All the world,' says Cuvier, in his recent analysis of Cordier's Essay, 'agree in believing that the mass of the globe has been in a liquid state. But whether it was aqueous or igneous liquidity, is a question upon which there is a difference of opinion.' The evidence of this original liquidity is derived, not merely from the records of geology, but follows as a probable inference from the spheroidal figure of the earth. The effect of its rotatory motion, while yet in a fluid state, as every philosopher knows, would be to flatten the polar regions, and elongate the equatorial. And although it might have been created with a spheroidal figure, yet this figure seems most naturally referable to that class of facts, which we may expect to find dependent on second causes. So far as a change in the earth, from a sphere to a spheroid, is concerned, however, either aqueous or igneous fluidity will equally well explain it.

It has heen thought a strong confirmation of the system of gravitation, that certain minute irregularities in the motions of the heavenly bodies, were first suggested by the theory, and afterwards confirmed by observation. M. Cordier has laid the foundation, by several of his suggestions, for a similar argument in respect to the theory under consideration. For example, he infers from his premises, that the crust of the earth possesses a certain degree of flexibility. Hence it would follow, that the operation of the tides must extend to the crust of the earth itself. Perhaps future observations may confirm this suggestion.

Again, the gradual refrigeration of the globe must produce a small contraction of its bulk. One effect of this process would be slightly to depress the surface of continents and produce an apparent rise of the waters. And effects of a similar kind have been already observed around the Baltic and the Mediterranean ; although requiring farther observations to fix their maxima.

La Place estimates that the length of the day has not varied one five-hundredth of a second during twenty centuries. But if the earth be gradually contracting, in consequence of its refrigeration, the consequence will be a slight increase of the velocity of its rotation, and a correspondent decrease in the length of the day. Another consequence of this accelerated rotatory motion will be, to render the spheroid more oblate. Effects so extremely small, however, can be detected only by the most accurate and long continued observations.

The mean density of the earth, as is well known, is five times and a half greater than that of water; that is, about twice as great as that of granite and most other rocks near its surface. At first thought, it would seem that this fact might be explained on the theory of central heat and fluidity, by imputing the great density of the internal matter to the enormous pressure exerted upon it. M. Cordier, however, is of opinion that this cause is not sufficient to account for so great a density. 'It is to be observed first,' says he, 'that fluids but feebly compress themselves ; that this compression has a limit, and that a very great heat balances the effects. Furthermore, the present lavas, after their coagulation, possess a mean specific gravity greater than that of the primary rocks taken together ; whence we may conclude, independently of every other consideration, that the density of the central materials of the globe results much more from their nature, than from compression. They would originally dispose themselves in the order of their specific gravities. The existence of gold and platina shows us that there may be found in the centre of the earth, substances having naturally a very great density.' *Translation*, p. 87.

With such views, the author thinks there is some probability in the hypothesis of Halley, which imputes magnetic action to the existence of an irregular mass, composed chiefly of metallic iron, which has a revolution of its own within the earth ; and he thinks that the phenomena of Saturn's ring, and the discovery of metallic iron in meteorites—circumstances unknown to Halley—increase this probability.

If this hypothesis be admitted, it will furnish a probable ground for determining the limit of the internal temperature of the globe. For the experiments of Newton, confirmed by those of Barlow, prove that iron, raised to a white heat, loses its magnetic virtue; although excessive pressure might very much extend the limit at which this annihilation would take place.

The geological reader will probably by this time begin to inquire, whether the theory under consideration has as felicitous an application to the details of geology, as we have seen it to have to the phenomena already brought under consideration. This is an important inquiry; since the facts which geology discloses must be in a great measure the result of this vast volcanic agency, which has been supposed to be the grand instrument in originating and modifying the crust of our world; and if a detailed history of the rocks, constituting that crust, contradicts the supposition of powerful igneous action, the theory must be given up, however beautifully it may explain other phenomena. It is the details of the rock formations, that have proved the touchstone, and the ruin, of former geological hypotheses. Some grand idea has been started, which seemed to furnish a satisfactory solution of some general or insulated facts; but when the geologist has carried it with him into the deep excavation, or up the lofty precipice, its fallacy has been soon manifest. To this test, then, let the theory under consideration be brought, and by it let its merits be weighed.

The limits of M. Cordier's work, however, do not permit him to apply his theory to the details of geology; though he makes a general application. On this account, we have placed the work of Mr Poulett Scrope at the head of this article. The greater part of it is occupied in examining the phenomena and the laws of volcanic action; but at the conclusion, he unfolds the elements of a new theory of the earth, corresponding essentially with that of M. Cordier. His work was published before that of Cordier, and he makes but little use of the experiments upon subterranean temperature, because this argument, before the appearance of Cordier's Essay, was developed only in a very imperfect manner. In the phenomena of the two hundred volcanoes, which are found constantly or occasionally active on the globe, and in the evidence which almost every part of the world presents of a volcanic action far more common and energetic in early times than at present, Mr

Scrope thinks he finds an adequate cause for the original formation of all the unstratified rocks of our globe, from the oldest granite to the newest trap rock; and for all those overturnings and irregularities among the stratified rocks, which meet the geologist at every step of his researches. He makes a distinction, however, between those explanations of geological phenomena, to which the mind is involuntarily directed by the appearances themselves, and those hypothetical considerations which relate to the original state of our planet, and to the earliest changes that took place upon and within it. If the merely hypothetical views should be found incorrect, it will not prove, for instance, that continents were not elevated, and granite and trap rocks were not produced, by volcanic action. We think this a wise distinction.

No man who has any correct geological knowledge of the continents of our globe, can doubt that they must formerly, and for a long period, have constituted the bottom of the ocean. The animal remains found in the secondary rocks of these continents, are, for the most part, marine. In a word, these rocks appear to have been slowly formed by subsidence, or imperfect crystallization; and the shells and other oceanic relics seem to have dropped to the bottom as they died, and to have become enveloped in the accumulating materials of the rock, which was subsequently hardened by heat, or by mere desiccation. The same process is now going on, though upon a diminished scale, in the ocean, and in fresh water ponds. Either, therefore, our present continents must have been raised by some internal force above the surface of the waters, or the waters must have subsided, so as to uncover them. There is no evidence of any such diminution of the waters. But on the other hand, the primary stratified rocks, all over the globe, are inclined, bent, and broken, just as they would have been, if some enormous force, acting in the direction of the principal mountain chains, had forced them upwards. The idea that the strata of rocks were originally deposited in an inclined position, and, especially, with the countless foldings and contortions which mica slate and grey wacke slate exhibit, is altogether preposterous, except perhaps in a few peculiar and limited cases. We are led then to inquire, whether there is any other evidence of a force within the earth, sufficiently powerful to produce so mighty a tumefaction. Two hundred volcanic craters reply with their bellowings; and some of them bring up before our eyes, from

the bottom of the ocean, islands of no inconsiderable extent; while a section of the strata composing the conical mountains thus lifted up, presents us with all the varieties of inclination, contortion, and disruption which we find in the rocks composing the general crust of the globe. In addition to all this, we see, on every side, evidence of the exertion of a volcanic power in early times, transcending by far its present feeble and intermittent action. How can we then hesitate to ascribe the elevation of continents to this same power. Such would be the occasional effect, we might presume, if the earth had been in a fluid, incandescent state within, while yet its crust was thin and partially consolidated.

It has been for five thousand years, and is still believed, by nearly all men, that the action of frost, rain, and streams of water has scooped out the existing valleys with which the surface of continents is grooved. This belief has been entertained, simply because men have neglected to compare together the cause and the effect. Had they done this, they would have seen at once the inadequacy of the former to produce the latter. Neither is the opinion tenable, that the last universal deluge formed most of these valleys; and for the same reason, namely, that the cause is disproportionate to the effect. But if volcanic power has raised our continents, the extensive and deep valleys which we usually find in mountainous districts, would be the natural consequence. For we find such valleys produced in those regions where this same agent is still in operation.

Have we any means of determining when all, or any, of the present continents, were raised from the ocean? The nature of volcanic action would lead us to infer, that this took place by repeated efforts of the power, with intervals of repose; just as we find earthquakes at this day, sometimes raising parts of the earth's surface by almost imperceptible increments. Occasionally, however, when local circumstances had enabled the repressive force to predominate for a long time over the expansive, a mighty '*paroxysmal*' effort of the power might have been sufficient to throw up a continent. But there is no reason to suppose that any event of this kind has taken place since man was placed upon the earth. The researches of Professor Buckland, as given in his *Reliquiæ Diluvianæ*, render it probable that our present continents, in great part at least, remain at this day as they were before the Noachian Deluge. If, however, it be true that outliers of the plastic clay and chalk

formation are found crowning the Savoy and Julian Alps, it would seem that this immense chain, and probably with it all Europe, were thrown up at a comparatively recent period. No facts within our knowledge lead even to a suggestion as to the epoch when the other continents were raised from the deep. Indeed, we place but little reliance upon any observations that have yet been made, in any part of the world, upon this point; although we do not see why the relative ages of different continents may not be as fairly within the reach of geological researches, as the relative ages of different rocks.

We think the argument in favor of the elevation of continents by an expansive force beneath them, to be so conclusive, that every class of geologists, Neptunians as well as Vulcanians, must yield to it. Nor do we see why the most thorough Wernerian might not admit such an origin of our continents, after the deposition of all the rocks from solution and diffusion in water. But those who believe in the igneous origin of the unstratified rocks, will be most likely to adopt this opinion. According to their views, it is the protrusion of granite and the trap rocks through the regular strata, that has produced the elevation of the higher parts of our globe, with all the confusion and overturning of their strata. The nature of these rocks, and especially their mode of occurrence, have produced this conviction as to their origin; and at this day it seems to be gaining ground rapidly among European geologists. In respect to the trachytic and basaltic rocks, indeed, we do not know of the geologist of respectability, who presumes boldly to defend their aqueous origin; and the same may be said, with few exceptions, in regard to all the trap rocks. If any are able to sustain the Wernerian opinion in regard to these rocks in the lecture room, before auditors who have never seen them except in hand specimens, we are confident, if they should follow the example of Daubuisson and Dauberry, by going forth to an examination of the trap rocks in their natural situation, they would return, like these able geologists, thoroughly converted. Or were they to sit down and delineate several series of regular horizontal strata, as they would be bent, broken, and elevated, by a mass of melted rock forced through them from beneath; and then compare this delineation with the section of the actual position of the basalt and stratified rocks of the Tyrolese Alps, as given by De Buch, and copied by Mr Scrope, they would be astonished, if not convinced, by the striking coincidence.

In regard to the older unstratified rocks, the porphyries, sienites, and granites, there is not yet so universal a belief among geologists of their igneous origin. And we apprehend that the more distinctly crystalline structure of these, than of the trap rocks, has produced more doubt on this point in the minds of geologists, than any other circumstance. For so far as their mode of occurrence is concerned, certainly there are no stronger indications among the trap rocks of protrusion from beneath, than among sienite, porphyry, and granite. But most of the crystallizations we witness, either in nature's or the chemist's laboratory, take place from solution in a fluid. And hence we are apt to refer examples of crystalline structures on a large scale, to this mode of production. But we ought to recollect that crystallization does likewise result from igneous fusion; sometimes when the substance is under strong pressure, and sometimes when it is free from pressure. The crystalline structure of the older unstratified rocks is, therefore, no argument against their igneous origin. But on the other hand the facts, that few, if any, of the ingredients of these rocks are soluble in water, except to a very limited extent, and that the weight of the water upon the globe is only one fifty-thousandth part of the weight of its solid materials, seem to be insuperable objections against the aqueous formation of the great mass of the globe.

But we cannot in this place enter into a detailed examination of the arguments on this subject. Our object is rather to give the views of Mr Scrope on some of the points connected with the origin of the unstratified rocks. His ideas of their original identity, at least of such of them as are crystallized, will appear from the following extract.

'It seems probable that ordinary granite composed of feldspar, quartz, and mica, was the original or mother rock, composing what has been spoken of as the general subterranean bed of heated crystalline rock, or *lava.*

'Circumstances accompanying its intumescence and reconsolidation, may be supposed in some cases to have converted the mica into hornblende, producing syenite.

'A great degree of comminution, occasioned by the friction of the crystalline particles on one another, may have sometimes reduced the granite to a porphyry; small particles of feldspar alone remaining visible in an apparently homogeneous base. A still further subdivision, either accompanied, or not, by some changes in the combinations of the elementary particles, may have given rise to compact feldspar (eurite or weisstein), or serpentine;

and the recrystallization of this latter rock, under favorable circumstances, to diallage rock.

'The extreme disintegration of syenitic granite, will, in the same manner, have produced greenstone, and perhaps the later traps.

'There are not perhaps any two of these varieties of crystalline rocks, which have not been found in nature passing into each other, either by sudden or gradual transitions.

'It cannot therefore be deemed a rash conjecture to suppose them all to have [been] derived from the same original; and it certainly appears most probable that the alterations they have undergone were the result of the circumstances attending their rise and protrusion towards the surface of the globe; since we have to guide us in this supposition the exact analogy of the congenerous crystalline rocks produced under our eyes by subterranean expansion, from volcanic vents, in which similar changes of mineral characters indisputably take place during the processes of emission and consolidation.' *Scrope*, p. 218.

The author supposes that granite, in some cases, was forced through the incumbent laminated strata, while yet in a solid state; but that in some instances extravasations of the highly ignited mass took place, filling the numerous fissures that must have been produced in these strata by the upward pressure. Where the intumescence was considerable, some variety of trap rocks would be the result. He also supposes that the lower laminated strata (gneiss, for example,) were sometimes driven upwards along with the granite, and variously replicated and broken. Hence the remarkable contortions which have been observed in this rock.

Thus far Mr Scrope seems to feel that he has proceeded on firm ground. The inquisitive mind, however, does not rest satisfied with these somewhat insulated deductions from geological facts, but is disposed to go farther back, and inquire into the origin of this internal ignition and fluidity of the globe, and especially into the mode in which the stratified rocks were produced and deposited; for in all the remarks hitherto made, it has been assumed that these were previously consolidated. The author is therefore tempted into a wider and more hypothetical field, giving us a 'Sketch of a Theory of the Globe.' It is ingenious and interesting; but our limits will allow us to present only a condensed summary.

The author supposes the earth originally to have been composed, at least to a great depth, of granitoidal matter in a crys-

talline state; and when it reached its present orbit, or before, (it having been, perhaps, struck off from the sun, as is supposed by Buffon and Laplace,) the enormous pressure under which it was held, and by which its crystalline structure was preserved, was partially or wholly removed. The consequence would be, a violent expansion of the external part, by which the water of crystallization would be suddenly turned into vapor, and this would carry upwards the disaggregated, and more or less liquefied crystals of the quartz, mica, and feldspar. The greater the expansion, the more caloric would pass into a latent state; and ere long the vapor must begin to condense and fall back towards the more solid parts of the earth.

'And in this manner, for a certain time, a violent reciprocation of atmospheric phenomena must have continued. Torrents of vapor rising outwardly; while equally tremendous torrents of condensed vapor, or *rain*, fell towards the earth. The accumulation of the latter on the yet unstable and unconsolidated surface of the globe, constituted the primeval ocean.' p. 229.

At so high a temperature, this ocean must have contained in solution, silex, the carbonates, sulphates of lime and magnesia, muriate of soda, magnesia, and lime; while large quantities of the upper disintegrated beds, particularly their mica, must have been suspended in the same fluid. These suspended matters must ere long have begun to subside upon the granitic nucleus beneath, the quartz and feldspar most abundantly, but carrying along with them some mica. This and the feldspar crystals would naturally arrange themselves so as to have their longest direction parallel to the surface on which they rested. This the author conceives to have been the origin of the gneiss formation.

As this primeval ocean continued to cool and to be less agitated by ebullition, some of the substances which it held in solution would begin to crystallize and mix with the sediment of suspended matter which would continue to deposit itself. Thus rocks would be formed, partly crystalline and partly mechanical; such as mica slate, quartz rock, and the transition slates. In some places, also, would the saccharoidal limestones result from the deposition of the carbonate of lime from solution. After a still farther reduction of temperature, gypsum and rock salt would be precipitated in a similar manner.

A solid envelope being thus at length formed of the stratified primary, and some of the transition rocks, a new process would

take place beneath it. The intensely heated nucleus of the globe would still continue to give off its caloric to the surrounding zones of rock, whose temperature had been greatly reduced by expansion. As they became heated again, the process of expansion would be resumed. But as the envelope of stratified rocks, which had formed on the surface of the globe, would oppose a resistance to the expansive force, this would go on accumulating, until the tenacity and weight of this envelope were overcome, when fissures would be produced, through which granite, in a solid or intumescent state, would be protruded, forming veins, beds, and overlying masses of granite, porphyry, sienite, and the traps.

In case the fissure did not open directly into the granitic matter beneath, and the temperature of its sides was sufficiently high, an expansion of these sides would take place, whereby the fissure would be filled with granitoidal rock, porphyry, or serpentine, thus giving rise to a particular variety of veins not uncommon.

If the temperature of the sides of the fissure was lower, aqueous vapor would exude, holding silex in solution, which would at length crystallize, along with other mineral and metallic matter sublimed from the lower part of the fissure, where the temperature was higher. This might have been the origin of many quartz and metallic veins.

No inference from geological facts is in our opinion more certain, or more generally admitted, than the one which supposes that, from the earliest times, very many violent and extensive inundations have taken place, by whose abrasive force, many particular formations, and the general surface of the globe, have been greatly changed. The formation of the various conglomerate rocks can be accounted for in no other way. According to the theory under consideration, these deluges commenced their ravages at the epoch of the first protrusion of the granite through the enveloping strata, and every subsequent disruption and elevation of this kind, was followed by a succession of these debacles.

'These sudden and partial elevations of the crust of the globe, and the other various causes which at this period disturbed the tranquillity of the primitive ocean, produced violent waves and currents, which broke up and triturated the projecting eminences of its bottom, and distributed their fragments in alluvial conglomerate strata, wherever the turbulence of these moving waters was

partially checked. The surface of the globe at this period consisted chiefly of mica schist; and hence mica and granular quartz predominate greatly in the conglomerates of this epoch; namely, in greywacke and granular quartz rock.' p. 236.

It is evident that such an event, as the elevation of a continent from the bottom of the ocean, must produce a tremendous rush of waters towards the antipodes. A reflux would follow this accumulation, not much less powerful; and thus an oscillatory movement would be communicated to the ocean, which would continue for a long time. Even during the earthquake of Lisbon, the sea rose sixty feet above high water mark at Cadiz, and along the whole coast of Portugal; and was affected on the coasts of England and Norway, and if we remember right, even in the West Indies. Similar effects have accompanied earthquakes in Peru and Calabria. And 'if elevations of but a few inches or feet of vertical height produced oscillatory movements in the ocean of such violence, what must be the effect of the sudden elevation of a mountain range like that of the Alps, from the bottom of the sea?'

During the time that the tide, produced by such a cause, remained at its highest or lowest mark, at any particular place, the waters would be comparatively quiet; and then the smaller particles suspended in it would be deposited, and rocks produced of a finer and more homogeneous texture. But when the rush of waters returned, it would bring along coarser fragments and produce conglomerates. Such a supposition will account for the very numerous and perfectly well defined alterations, which we so often see in a sandstone formation, of the finest shales, and the coarsest puddingstones; a fact which we have always deemed more difficult of explanation than almost any other in geology.

The elevation of the primary strata by the protruding granite would probably raise some part of them above the level of the ocean, and consequently afford a residence for animals and vegetables. Mr Scrope supposes that some of the marine animals, of a simple structure, might have lived in the ocean while its temperature was yet no lower than the boiling point of water. In the rocks subsequently formed, therefore, we should expect to find these vegetables and animals imbedded; as in fact they do occur in most of the secondary and tertiary strata. These strata he supposes to have been formed by a succession of occurrences, such as we have mentioned. That the unstrati-

fied rocks have been ejected at different and very numerous epochs, no one will doubt who has examined them in different localities, and who believes them to have an igneous origin. And that their protrusion would produce those tumultuous agitations of the waters which we have described, will be equally evident to every reflecting mind.

'It appears to me therefore on the whole,' says the author, 'that the formation of the grand mineral masses of every age, composing the known crust of the globe, is attributable to *three* primary modes of production, distinct in their nature, but of which the products have been mingled together, from circumstances of isochronism or collocation. These are,

1. The chemical precipitation of various mineral substances; but particularly silex and carbonate of lime, from a state of solution in the ocean, or other body of water; as its temperature and solvent powers gradually decreased.

2. The subsidence of particles of mineral matter, of various degrees of coarseness, from a state of suspension in the ocean or other reservoir, into which they had been taken up, either by the violent escape of aqueous vapor from the interior of the globe, by the abrasive force of marine and fluviatile currents, or finally by the decomposition of the shells of molluscous animals, which possessed the faculty of elaborating their coverings from the substances they procured from sea water.

3. The elevation of crystalline matter through fissures in the crust of the globe, which had been already formed in the two former modes; this rise being occasioned either by the expansion of a lower bed, in which case the rock was elevated nearly in a solid state; or by its own intumescence, owing to a sudden diminution of compression; in which case the matter rose in an imperfectly liquid state, and at a high temperature.' p. 241.

Geologists have generally been persuaded, that no cause now in operation is adequate to account for all the phenomena which their science discloses. Cuvier, especially, in perhaps the best Essay on the Theory of the Earth that ever was written, asserts, as the result of his examinations, that 'none of the agents nature now employs were sufficient for the production of her ancient works.' But Mr Scrope, not without reason, considers it a very favorable circumstance for his theory, that all the modes by which he supposes rocks to have been produced, are still employed by nature for the same purpose. Speaking of these modes he says,

'They have one immense advantage over most, perhaps over all, of the hypotheses that have been as yet brought forward to explain

the same appearances; and which speaks volumes in their favor; and this is, that *they are still in operation;* with diminished energy, it is true, but this is the necessary result of their nature.

'The first mode still gives rise to calcareous and siliceous rocks of great solidity, and even of a crystalline texture, in the vicinity of certain thermal or mineral springs.

'The second still produces strata of marls, sand, and gravel, at the bottom of the sea, of inland lakes, and in the beds of rivers; which strata bear a very decided analogy to the earlier sandstones and limestones.

'The third is in constant operation wherever volcanoes break out into activity, or earthquakes produce elevations of the solid strata.' p. 242.

The author rejects the Huttonian doctrine, that the strata, which were deposited at the bottom of the ocean, were subsequently consolidated by the internal heat; and he rejects this notion for what has always appeared to us a most satisfactory reason, namely, the occurrence, in many instances, of indurated strata above clay and shale. He supposes that crystallization and desiccation will account for the consolidation of the strata.

And here we would suggest an important general distinction between this theory of Scrope and Cordier, and that of Hutton and his followers. The latter assumed the existence of an intense central heat, because this would most rationally explain the appearances presented by the earth's crust. The former endeavor in the first place to demonstrate the existence of this internal heat, from the phenomena of earthquakes and direct experiments on subterranean temperature; and having ascertained the laws by which its operation is regulated, they infer that it must, in the nature of things, have elevated continents and produced all the observed phenomena of inclined and contorted strata of veins, faults, slips, &c. Mr Scrope's attempt to carry us back to the origin of this course of things, must be regarded, like the theory of Hutton, as merely hypothetical.

We have seen that the advocates of central heat derive their evidence of its existence from two principal sources, volcanoes and subterranean temperature. Mr Scrope's conclusions are founded chiefly upon the former, and those of M. Cordier chiefly upon the latter; neither of them, however, neglecting collateral evidence. A learned geologist in this country, who 'admits the igneous fusion of our planet,' expresses the opinion, in a cotemporary Journal, that the best evidence of this fact consists in the spheroidal figure of the earth, and in the ejection

from volcanoes, in a state of absolute fusion, of glassy obsidian, pumice, trachytes, pearlstone, &c. *from beneath the granitic crust* of the globe. Sir Alexander Crichton, in the 'Annals of Philosophy' for November and December, 1825, infers the central heat from another very curious fact in geology, which we have not yet noticed.

The impressions of plants, found so abundantly in the coal formations of the most northern latitudes, were, in the opinion of all able botanists, the products of regions as warm at least as those between the tropics at the present day. And even in the latest formations, in England, are found remains of the cocoa nut and other analogous vegetables; while the remains of land animals, which Mr Buckland has shown were inhabitants of the same country immediately previous to the last general deluge, are mostly of such species as at present exist in tropical climates; and the same is true of nearly all the northern parts of Europe and Asia, and we might add, of North America. It is even asserted, that the older the rock in which these remains are found, the more decidedly analogous are they to plants and animals of the torrid zone; thus indicating a gradual diminution of temperature since the deposition of the oldest secondary rocks. From these facts Sir Alexader Crichton infers the existence of an internal heat, gradually diminishing from the creation to the present day.

His explanation of the origin of this heat is interesting, because it developes the germ of another new theory of the earth, which has been recently broached, and is at this moment maintained by several very able geologists.

'The nucleus of the globe consists,' says he, 'of the metallic bases of the earths and alkalies, which in the beginning of things took fire from the contact of air and water, and produced, by their combustion, granite. The latter retaining its temperature for a very long period, would impart to the earth a source of heat independent of the solar rays, which must have gone on progressively diminishing down to the present time.' *Dauberry on Volcanoes*, p. 431.

Without stopping to array objections against this hypothesis, we would remark only, that while the facts which we have stated in regard to organic remains in northern latitudes naturally suggests the idea of central heat, they are most satisfactorily explained by the theory of Scrope and Cordier. The former just alludes to the subject; but, to our surprise, the lat-

ter does not even mention it, although it seems one of the most happy applications, and strongest confirmations of his theory, which can be exhibited.

Our readers, however, will expect that the new theory of the earth, adopted by some other geologist, and to which we have already more than once alluded in our remarks, should receive a moment's attention. This theory took its rise from Sir Humphrey Davy's discovery, a few years since, of the metallic bases of the alkalies and earths. Professor Dauberry, in his recent very able and interesting work on Volcanoes, has adopted it, so far as it is applicable to their explanation. But the editor of the 'American Journal of Science,' in his review of Dauberry's work, has given, with his characteristic perspicuity and elegance of diction, the most full and graphic description of the hypothesis which we have seen; and he has somewhat modified and extended it, by some peculiar views of his own. He thus describes the origin of the alkalies, earths, rocks, oxides, the sulphurets and phosphurets, the acids, salts, &c.

'If we suppose that the first condition of the created elements of our planet, was in a state of freedom, the globe being a mass of uncombined combustibles and metals; when the waters, the atmosphere, and chlorine, and iodine, and perhaps hydrogen were suddenly added, it will be obvious from what we now know of the properties of these elements, that the collision would awaken dormant energies, whose first operation would be a general and intense ignition, and a combustion of the whole surface of the planet. Potassium, sodium, and phosphorus would first blaze, and would immediately communicate the heat necessary to bring on the action between the other metals and combustibles in relation to the the oxygen and chlorine, and in relation to each other. Thus a general conflagration would be the very first step in chemical action, and life not having yet dawned on the planet, this conflagration would be the step most admirably fitted to prepare the globe for the living beings by which it was to be peopled.

'In such circumstances, there would also be great commotion; steam, vapors, and gases would be suddenly evolved in vast quantities, and with explosive violence; the imponderable agents, light, heat, electricity, and magnetism, and attraction, in various forms would be active, in an inconceivable degree, and the recently oxidated crust of the earth would be torn with violence, producing fissures and caverns, dislocations and contortions, and obliquity of strata; and it would everywhere bear marks of an energy then general, but now only local, and occasional. It is, however, obvious, that this intense action would set bounds to itself; and that

the chemical combinations would cease, when the crust of the incombustible matter thus formed, had become sufficiently thick and firm, to protect the metals and combustibles, from the water and the air, and other active agents.' *Am. Jour. Science, Vol.* XIV. p. 88.

In order to explain volcanic action at the present day according to their theory, it is supposed that occasionally water percolates through this oxidized crust of the globe, penetrating to the metallic nucleus beneath. This, it is supposed, would produce intense ignition, and numerous decompositions; from which would result all the phenomena of volcanoes.

Mr Scrope thinks that his theory 'is not incompatible with the idea of the granitic involucrum of the globe having been produced by the superficial oxydation of a metallic nucleus,' agreeably to the above hypothesis. He thinks, however, that this hypothesis 'smells a little of the laboratory.' Now we do not feel as if this were any objection to it. Indeed, had geological theories exhibited more evidence of having been brought to the test of the laboratory, and of having been more thoroughly digested in the retort and the crucible, that is, had they been more frequently founded upon experiment, and less upon conjecture, we think they would have been more permanent and less extravagant. We confess, however, that we are not converts to the above hypothesis; and one of our difficulties is derived from the laboratory. Since it was proposed, Berzelius has succeeded in obtaining silicium; and he finds that 'in its densest state' (the state in which it would probably have existed originally in the earth), 'it may be made incandescent in the air without burning; and it does not undergo any change in the flame of the blow-pipe.' And indeed, none of the metals, except potassium and sodium, will take fire by contact with water. Now silex constitutes probably three quarters of the solid crust of our globe; while potassa and soda cannot form the fiftieth part; and even if we take into the account the alkaline salts in the ocean, we must be permitted to doubt, whether the quantity of potassium and sodium could have been sufficient, in the beginning, by their combustion, to have raised the temperature of the immense mass of the other metals, with which they were mixed or alloyed, sufficiently high to produce their oxydation.

So far as this hypothesis is applied to the explanation of volcanic phenomena, we must regard it as peculiarly unsatisfactory. If it be admitted that water, in the first instance, could have

penetrated through the oxidized crust to the metallic nucleus beneath, and thus have commenced volcanic action, how, after the surrounding matter had become so intensely heated as the products of volcanoes shows us it must be, could water continue to find its way, without being vaporized, to the same spot, year after year and century after century, so as to produce a constant eruption of matter from the same crater; as takes place in such volcanoes as Stromboli and Kairauea?

The Editor of the American Journal of Science has, however, suggested a very ingenious addition to this theory. He supposes the different layers of metals and other substances in the earth, to constitute a vast galvanic battery, in ceaseless action, whose igniting and decomposing power is inconceivably great. This, in connexion with the causes we have mentioned above, or even alone, he conceives to be sufficient, satisfactorily to explain the phenomena of earthquakes and volcanoes. The novelty and grandeur of this idea would incline us to adopt it, did we not feel that the supposition of central heat and fluidity was more in accordance with facts, and less incumbered with difficulties.

But we must hasten to a conclusion. Our principal object has been to present our readers with a summary view of the present state of geological theories, particularly of the theory of igneous internal fluidity, with its applications to the solution of phenomena. And here would we say, in the words of Cuvier, in his analysis of Cordier's Essay already referred to, that 'these conclusions, so important and various, and many others, which the space allotted to us does not permit us to develope, result from a fact, very simple in appearance, but the fecundity of which is indeed wonderful; namely, the sensible increase of temperature at those comparatively small depths to which we are able to penetrate, and the very probable supposition that this increase continues proportionally to the greatest depths.' If the fertility of its applications affords any evidence in favor of the theory of gravity, as is generally thought, we do not see why the same argument may not be urged in favor of the theory under consideration. We need not think it strange therefore, that it should forcibly arrest the attention of philosophers, and indeed of all intelligent men; since the grand arguments on which it rests, are intelligible to them, although they may not be familiar with the technical parts of science. Indeed, if we do not greatly mistake, this is rapidly becoming the

prevalent geological theory of the day,—certainly on the other side of the Atlantic. All decided Huttonians, and most of those who have been thorough sceptics in respect to all geological theories, will be very apt to give to this their partial or entire assent. And as to the Wernerians, we think that some indications will justify us in the prediction, that most of them will ere long adopt the other ingenious theory which we have explained, derived from the discovery of the metallic bases of the earths and alkalies. We think, that for a time at least, these two theories are destined to divide the geological world; and we should regard such a change of opinion as an important step towards a unity of views among geologists; since these theories are certainly not so diverse from each other as those of Hutton and Werner.

We have no desire to conceal our present partiality for the theory of igneous internal fluidity. We have been long among the number of confirmed sceptics on the whole subject of geological hypotheses. But we have become wearied with hovering so long over what seemed to us the shoreless ocean of uncertainty and conjecture. We see now something that looks like *terra firma;* and we are willing to try, if we cannot there find a little rest for the sole of our foot. It may prove a Delos; if so, we have only to spread our wings again, and wait for the emergence of the true Ararat. But we would by no means have it understood that we are so committed, in the defence of this theory, as to be determined to sink or swim with it. The history of past theories of the earth admonishes us that too much scepticism in respect to them is safer than too much credulity. The present theory we think explains five phenomena where any other explains one, and therefore we prefer it to any other; still, it requires a continuance of experiments and observations to establish it as firmly as the Newtonian doctrine of gravitation, and we shall welcome any successful attempt to substitute another theory which may be more substantial.

In the application of the igneous theory, that has been examined, we have confined ourselves to this globe. M. Cordier, however, has hazarded some bold and original remarks, upon the extension of the same principle to other worlds; and indeed to the whole material universe. He has sustained these conjectures by no facts; yet in reflecting upon the subject, it has seemed to us that some circumstances, relating to the heavenly bodies, might lead an advocate of this theory to conjecture

that changes, like those supposed to have taken place within our globe, may be going on in other worlds. We will just allude to these, in order to excite the attention of the learned to the subject.

It has been conjectured that the four small planets, revolving between Mars and Jupiter, are the fragments of a large one, which once revolved in nearly the same orbit, and was subsequently burst asunder by some powerful internal force. If such a refrigerating process has taken place in the other planets, as this theory supposes to be going on in the earth, might we not presume, that, under possible circumstances, such a terrific explosion might take place?

It has been thought by some astronomers, that volcanoes exist in the moon. If so, their origin is probably similar to that of those existing on this earth.

Those fragments of solid meteors, that are not unfrequently projected to the earth, bear evident marks of fusion. Indeed, the whole body of the meteor, when near its perigee, appears usually to be in a state of ignition.

The sun itself, what is it, but an immense globe of liquid fire? Such certainly does it appear to be, as seen through the most powerful telescopes. And its spots, what are they but an incipient crust, beginning to form over its surface, but which is merged again, after a certain time, in the fiery, and perhaps agitated ocean beneath, which is not yet sufficiently cooled to allow its complete formation? And the zodiacal light, what is it, but the elastic vapors which are driven from the sun's intensely heated mass, and which, partaking of the sun's rotatory motion, have assumed a figure so oblate as to become almost lenticular.

The appearance of comets is extremely chaotic. The nebulosity that surrounds them, becomes more dense towards their centres; and in some instances distinct *nuclei* are visible; but out of sixteen, which Dr Herschel examined with his powerful telescope, he could discover *nuclei* in only two; and he actually saw the fixed stars directly through the centres of some of them. Notwithstanding this circumstance, this astronomer is of opinion, that these bodies are self-luminous. Where the nucleus was visible, it had a well defined, circular disk, shining in every part, without any of the defalcation which it ought to exhibit, did it shine by the reflected light of the sun. Now, who, that has read Mr Scrope's hypothesis of the manner in

which the crust of the earth was first formed over the liquid, fiery mass, does not see, in this description of comets, a close resemblance to the state of our globe, when its ignited materials, liberated from their pressure, were expanding, and immense volumes of vapor and gas were rushing outwards and forming a vast atmosphere, self-luminous through the ignited particles carried up with it, or by the coruscations of the electric fluid. In some of the comets, the ignited central mass is visible; in others, of less size, all the materials seem to have been dissipated, so that the stars are visible through them. Nevertheless, on Mr Scrope's theory, we can predict that even these may yet become solid, habitable globes; and who knows, but that when they are prepared for inhabitants, the fiat of the Almighty may cause them to revolve in circular orbits, like the planets, around some distant sun, and fill them with new races of happy beings!

There appears to be a great resemblance between comets and nebulæ; and indeed, Dr Herschel, in some of his latest remarks on the subject, 'concludes them to be peculiar condensed matter, and supposes that they may constitute, or become, comets.' We might, therefore, apply to these nebulæ, remarks similar to those which we have made upon comets.

Thus much in illustration of the sage maxim of Hermes; *συμπαθῆ εἶναι τὰ ἄνω τοῖς κάτω, there is a correspondence between things above and things below.* We close with the concluding paragraphs of M. Cordier's Essay.

'We shall now be permitted to repeat, that it is not through the spirit of system that the notion of a central fire is restored. It is in spite of system; in spite of many prejudices. This revolution of opinion is produced by the influence of facts. It results from diligent study, profoundly devoted to phenomena of very different kinds. We cannot, in particular, believe, that it is by chance, that natural philosophy, astronomy, and geology arrive at the same conclusion in following routes so different. We can say, therefore, without fear of advancing too far, that the hypothesis, of which the sciences seem to stand in equal need, already presents the characters of a substantial, fundamental principle; and every thing presages, that it will have an influence on the theory of the earth, as powerful as that of the great principle of gravitation upon the theory of the motions of the heavenly bodies.

'If it is proved that the earth is not an inert mass, as has for a long time been supposed; if the appearance of inertness is owing to the tardiness of the phenomena, and to their feeble intensity;

if all is laboring and moving within, as all is labor and movement without, we arrive at a result of the highest importance, since it seems applicable to all the celestial bodies; and thus we obtain stronger proof of the existence of a great principle of *universal instability*, which was announced, or dimly seen, by Newton and other philosophers; a principle, superior to those grand rules, which we have been accustomed to regard as constituting exclusively the laws of nature, from the security which we see in it, above the longest and apparently perfect revolutions of the solar system; a principle, which appears to rule the universe, even in its smallest parts; which incessantly modifies all things, which changes, or misplaces them, and without return; and which carries them along, through the immensity of ages, to new ends, which human intelligence cannot certainly penetrate, but of which it may nevertheless be proud to have foreseen the necessity.' pp. 90–92.

Art. II.—*The Substance of Two Reports of the Faculty of Amherst College to the Board of Trustees, with the Doings of the Board thereon.* Amherst. 1827.

The subject of education has of late excited so much of the public attention, that no apology will be required by our readers, if we occasionally introduce it to their consideration. It is well known to all, that the system of education in this country, in all its departments, has been for some time the object of severe scrutiny; that new text-books have been multiplied in all branches of study, and new views of instruction been adopted by teachers; that seminaries have been founded on new plans, and that, in their zeal for improvement, our older institutions have not been able to keep pace with the desire of reform which pervades the community, and are now meditating important changes. We avail ourselves, therefore, of the opportunity afforded by the public notice, which the Faculty of Amherst College have given of an innovation on the long established usages of the American colleges, to offer our views on some of the changes proposed in our system of collegial education. In the remarks which we shall make, we design nothing unfriendly to an institution which is supported by a large amount of influence. We propose to discuss a

ERRATUM.—On pages 279, 287, and 288, for *Dauberry*, read *Daubeny*.